eastward velocity which it possessed when it left the equatorial regions. It is a matter of indifference in what way this energy is consumed by the molecules of the water, whether it be in friction in rotation, or whether it becomes potential in the raised water through which the current flows; for in either case it is the resistance offered by the stationary molecules which causes the moving molecules to lose their velocity. The resistance being molecular, that which holds true of eastward holds equally true of westward motion. This is proved also by the fact that a current flowing from a higher to a lower latitude has its westerly motion due to rotation as effectually checked and diminished as a current flowing from a lower to a higher latitude has of easterly. And what holds true of motion to the east or to the west, holds equally true of motion to the south or north, for there is no reason why the resistance should be less in one direction than in another.

It therefore follows that it is impossible that 6 foot-pounds could impel a pound of water from the Equator to latitude 60° against the molecular resistances to its motion, when during the passage of the pound of water it requires 9,000 foot-pounds to overcome the resistance to the easterly deflections which take place. Or if the molecular resistance of water be so infinitesimal that 6 foot-pounds is sufficient, then it is impossible that molecular resistance could consume 9,000 foot-pounds during the easterly deflection which takes place.

I respectfully submit that this is a clear and obvious demonstration of the mechanical impossibility of the gravitation theory of oceanic circulation.

Prof. Everett says that Mr. Ferrel's argument from the tides is quite conclusive in showing that the forces arising from difference of temperature are of sufficient magnitude to keep up an oceanic circulation. If. Prof. Everett, like Mr. Ferrel, really supposes that a slope produced by the moon is the same as one produced by difference of density, and that the process by which the water tends to regain its level is the same in both cases, I am not surprised he should consider Mr. Ferrel's argument conclusive.

I beg to refer Mr. Wallace to the *Philosophical Magazine* for October 1871, p. 244, for an explanation of the fallacy of Dr. Carpenter's famous experiment to which he alludes.

Had the present state of my health permitted, I should have entered somewhat more fully into some of the above points, but in the meantime I must withdraw from any further discussion.

Edinburgh, August 27 JAMES CROLL

The Aurora of Feb. 4

THE Hon. Rawson Rawson, Governor of Barbadoes, has favoured us with the following note:—

"Memo. for his Excellency the Governor-in-Chief.

"The aurora of the 4th of February last, to which you allude, and notices of which you kindly sent me in NATURE, was seen here, and caused much concern.

"I was not myself an eye-witness, but I have descriptions of it from trustworthy persons. I was first informed of it by a servant of mine, who has the overlook of Mont Grace.* He was returning to his home near the Fort after 7 P.M., when, about a mile away from Mont Grace, his attention was arrested by what he imagined was a 'great fire'—the trees at Mont Grace and all about the yard were lighted up and clearly seen. My brother, who leased a sugar plantation in the neighbourhood of Government House, and Mr. Gordon, who owns one in that immediate neighbourhood, happened to be both in Scarborough at the time, and, seeing the 'great glare,' were both seized with the notion that their respective properties were on fire, and hastened out to them to find that the supposed fire was farther away.

"Mr. Taylor, master of the barque *Tobago*, was riding at anchor at Courland Bay,† and was a witness of this aurora. He described it to me as of 'a dark-red colour, extending half way up to the zenith, and very brilliant, its situation being about N.W. by W.' The labourers exclaimed that St. Vincent was on fire.

"This aurora lasted till half-past nine. Such a phenomenon, if not altogether unknown in this latitude, is at any rate very rare.

"Tobago." "DUGALD YEATES"

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* Mr. Yeates' property, three miles from Scarborough, Island of Tobago. (Tobago is in 11° 9′ N. lat., and 60° 12′ W. long.)

† On the opposite side of the island, *i.e.*, the northern side.

The Solar Spectrum

I HAVE lately obtained and read "Schellen's Spectrum Analysis," translated by the Misses Lassell, and edited by Mr. Huggins, and feel at length constrained to dissent from a statement which I there find—in this the present standard work on the subject—distinctly and repeatedly made, as I have seen it made elsewhere before, a statement belief in which has tended and must always tend to deter many from prosecuting independently a most interesting study. I refer to the passage beginning "The Possibility of Observing" (p. 382) to end of paragraph, italicising the words "ordinary" in l. 7 of p. 383 ' by increasing the number of prisms" three lines below, "highly dispersive power" in line 22.] The italics are mine, and are intended to indicate that to which I object, not that the particular passages in which they occur are explicitly incorrect, but that they implicitly convey the incorrect notion, that the "highly dispersive power" is essential to the primary success of the observation "of the lines of the prominences in bright sunshine."

The reason of my objection will be found in the following extract from an unpublished letter dated May 3, 1869:—

"I think it will surprise you to hear that I have just seen Mr. Lockyer's three bright solar lines at several parts of the sun's circumference with the Royal Society's telescope and spectroscope *without any appliances or devices whatsoever (sic)*, and that with the greatest ease and certainty. Had I merely *looked* for them, or for anything of the kind, a twelvemonth ago, I do not see how I could have failed to see them!

"When the slit is placed parallel to the limb, the red line is vivid across a bright solar spectrum, and the line near D (there is no doubt about its position when seen in connection with the solar spectrum) which is less prominent, as also the line at or near F, are easily seen."

Also in another letter of the same date:—"Before I went into camp last December (while still rumours only of Janssen's observations were current) I resolved to try with coloured glasses. They were not received till too late; the instrument was packed up, and I was away. By the time I returned the question had passed on; but I still wished to carry out original intentions, and prepared accordingly, and was proceeding to direct the telescope this morning when I saw the red line in the undefended part of the slit, where I was focussing on the sun's limb. Of course, I saw at once that I could do without my coloured glass, which, practically, limited my field of view, and accordingly removed it, and examined various parts of the limb with no screen at all. At nearly all I could see the same three lines. At one place the red was so bright that an outsider looking in at the spectrum would certainly have carried away the impression of a coloured ribbon with a bright line of red near the end." As a matter of fact my wife had no difficulty in seeing at any rate the red line.

Now, the application of this is to be found in the fact that the spectroscope in question contained a single equilateral prism and no more.

Were there any merit in a fortuitous discovery of the kind, it would suffice for me to declare that I was in complete ignorance of the methods and appliances by which MM. Janssen and Lockyer had succeeded in doing long before that which I now found so obvious. What I do insist upon is that the visibility not of the prominence *lines* only (see also Proc. R.S., No. 113, 1869, in this connection), but of the prominences themselves does *not* require a high dispersion. I have a fair acquaintance with prominence forms; but it has been derived almost entirely from a study of them with *an open slit*, the use of which I had learnt long before, and a *single prism* spectroscope.

Of course, I do not contest that the power of extended examination depends directly upon increase of dispersive power, only that the *lower* limit is fully attained by a single 60° prism.

That high telescopic power is not essential either is proved by the fact that I have examined prominences by applying a 1¾ in. object glass to the end of the sliding tube of the spectroscope at solar-focal distance, and using the instrument on its "soirée" stand on a table—by way of experiment—with an amount of success which, in 1868, would have made some sensation.

It is obvious to remark in reply that probably the explanation of the ready visibility asserted is to be found in more favourable climatic conditions. I cannot admit it. By night, it is true, there is often a remarkable translucency; but the dusty, agitated state of the atmosphere (in May) under a tropical sun, and at an inland station, can certainly not be considered favourable for observations of this particular character. And even were such

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an explanation in some measure correct, clear skies are not so unknown in Europe and America (nor indeed in England) that the illumination of the atmosphere can be broadly said to "completely overpower" that which "in an ordinary instrnment" is by no means overpowered in India in ordinary states of the sky.

The erroneous notion to which I refer has been promulgated again and again. It is unnecessary that I should specify the various passages—in lectures and elsewhere—and ultimately in Mr. Proctor's work on the Sun*: but having now found it so distinctly enunciated in the above cited passage, and elsewhere, in "Schellen," the time seemed to have come when it ought no longer to pass unnoticed; the more so as I have never been able to understand the real reason why the momentous discovery was not made earlier. It has been said that *knowing where to look*, the main difficulty was overcome. But two days after my first experience of these three known lines, I recognised the presence of three more hitherto unknown ones—and subsequently of a seventh. From that time to this however I have not seen any others, with the same dispersive power.

If asked how it was that, with the very same power at command, I had not myself seen them before; I need only reply that I had small leisure by day, and was under the impression that the experiment had been fruitless in more experienced hands—the identical reason which P. Secchi has given for a like remissness, in this very matter.

Bangalore, Aug. 25 J. HERSCHEL

Botanical Terminology

I VENTURE, as no one else has done so, to make a few remarks on Mr. Kitchener's letter.

I suppose the necessity will not be denied of employing some technical terms in studying subjects which do not fall under ordinary observation, and for the discussion of which ordinary language is consequently insufficient. When these technical terms are first devised, it is natural, indeed unavoidable, that they should reflect the scientific ideas current at the time. But inasmuch as knowledge progresses, we find ourselves, sooner or later in every branch of science, in the predicament of having to give effect to new views in terms which are an inheritance from old ones. We are able to do this because things themselves remain the same though our ideas about them change, and the names they once received with an intelligible meaning have now become purely arbitrary. No man bearing the name of, say, Baker, would probably change his name because he did not make bread. Nor do chemists discard the term oxygen because there are acids of which it is not a constituent. In the same way the morphological analogy implied in the use of the term "ovule," in the case of plants, is undoubtedly incorrect, but any one must have a singularly tender conscience who would object to it on that ground.

To save, therefore, confusion, and preserve uniformity in scientific literature, there is a tacit convention to treat in a great many cases as arbitrary terms words which once implied acquiescence in a theory. That a word in common use belongs to "a pre-Adamite stage of botanical knowledge," as Mr. Kitchener calls it, is not, I take it, sufficient ground for replacing it with another if there is no ambiguity in its application.

Next I would remark that Mr. Kitchener appears to me to have an exaggerated notion of the copiousness of botanical terminology. The number of terms really indispensable is not large. For example, he speaks of the troop of words ending in "tropous." Was this particular noun of multitude suggested by the termination? because as a matter of fact the troop consists of three. Prof. Henslow found no difficulty in teaching the terms contained in Prof. Oliver's Lessons in Elementary Botany to girls in a village school. Surely the Rugby boys cannot be less apt.

That Professor Henslow succeeded seems to dispose of the objection that a knowledge of Greek is "a necessary *open sesame* to the correct remembering and spelling of botanical terms." To teach these terms as "unintelligible gibberish" is only what in any case must be done with whole hosts of words not very different in form. Why should it be insuperably difficult for a boy, even if ignorant of Greek, to remember spell and apply the term hypogonous when he cannot possibly evade sometime or other having to face hypothesis, hypochondria, and hypocrisy, to say nothing of hydrostatics, hydraulics, hydrogen, and hydrocephalus?

I can see no reason why, as Prof. Henslow was in the habit of doing, technical terms carefully reduced to the smallest number absolutely required (and text-books bristle with unnecessary ones) should not be taught to boys as mere arbitrary names. Syngenesious, as a mere matter of taste, seems to me preferable to "united by dust-pouches."

If this be done, Mr. Kitchener's further difficulty as to "gamogenetic analogy" disappears.

The teacher, of course, may himself reasonably exercise some liberty. Thus no one would, I suppose, object to quincuncial being expressed by $\frac{2}{5}$, though quincunx is to be found in any dictionary, and is a word for which botanists are not responsible. Again, the suggestion to express by a fraction the depth of leaf-incision is really commendable, even to technical descriptive botanists.

October 1 W. T. THISELTON DYER

The Hassler Expedition

UNDER this heading in your number for August 29, p. 354, is this sentence, "One lesson I must confess to having learned at Indefatigable Island (Galapagos). I saw there indisputable proof that the surf of the sea is capable of rounding angular fragments of lava into pebbles somewhat resembling in shape (but not at all in polish and grooving) glacial boulders. I had always from boyhood doubted the power of the sea to make angular fragments round. I had supposed that the action of the surf upon such fragments would be simply to pack them into a sort of McAdam's roadway. And even now, having had the proof that under peculiar circumstances the sea can make a tolerable imitation of drift, I am not a whit more ready to believe that the sea made the drift itself. You may prove to me experimentally that flour can be made from wheat with a pestle and mortar, but that will not convince me that the flour markets of the world are thus supplied."

If the countless myriads of tons of beach on the shores of this globe could be passed through the hands of this writer, he would not detect a single "angular fragment" (McAdamised) among them. On the shore each lump of rock is successively worn into a boulder, each boulder into a pebble, and finally each pebble into sand. This is the main source of the sand which lies between the beach and the ooze-bed of the ocean.

But the sea-shore factory of boulders and drift is not the only factory, or even the largest factory of boulders and drift. The rocky gullet is the main boulder factory. Lyell (Principles), speaking of Etna, attributes "the enormous rounded boulders of felspar, porphyry, and basalt, a line of which can be traced from the sea from near Giardini, by Mascali and Zapharana to the Val del Bove" to one flood of melted snow. The valleys of the low part of Teneriffe, away from the Peak and near Santa Cruz, are almost all dry except in rain. The beds of the upper parts of these valleys are sheer rock, the middle parts wear the appearance of torrents of boulders, the lower parts are *alluvial plains* of boulders, and opposite the mouths of these valleys are very commonly deltas and bars of boulders. Behind these bars, after each rain, large deposits of earth and sand are formed which the people collect diligently. Where permanent streams exist, they are usually lost at a considerable distance above the mouths of the valleys. That is, except in rains, they percolate to the sea beneath the plains deltas and bars of boulders.

From the sides, hundreds or thousands of torrents of boulders fall into these rivers of boulders. Sometimes these lateral shoots have formed barriers of boulders across the main valley behind which large beds of boulders and earth have accumulated, again to be cleared out and thrust down to the sea-shore by heavy longitudinal rain floods.

So, in Madeira, who does not know the sea-shore boulders of the Praya-Formosa? and for fresh water boulders, the stream at Funchal brings down such a crop at every flood as to choke the channel through its delta of boulders, and unless the channel is kept clear of them artificially, the lower town is subject to the most disastrous inundations.

I mention Teneriffe and Madeira because, like the Galapagos, they are deep-sea volcanic islands. Their surfaces have been ejected when they were already above the sea, and they have been coated and re-coated thousands of times by floods of melted rock when they had long been *sub dio*. So that I conclude that even Agassiz would not attribute the moulding of their surface to the "Glacial epoch." But leave volcanic islands or volcanic mountains out of the question, there is not a mountain stream or

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* See particularly p. 286, footnote.

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